

CANCER RESEARCH INSTITUTE  
NEW ENGLAND DEACONESS HOSPITAL  
194 PILGRIM ROAD  
BOSTON 15, MASSACHUSETTS

Professor Joshua Lederberg

January 25, 1956

P.S. I see from your second letter that you are already familiar with Gunn's review. We are playing with the atropine esterase problem. Thanks for the additional references and comments.

We used Weiss' term "molecular ecology" because it caught our fancy as a way to express the multi-variant chemical system comprising the metabolic state. It would be a mistake to limit its reference to the molecular surfaces, even in the original context, as Weiss did. It is, of course, a (fuzzy-) blanket term.

There are few if any precise measurements comparing activities in whole cells (slices) and extracts of animal cells. However, these are stages at which many activities have been studied in the course of learning about the enzymes. There are consequently roughly comparable data for many systems, such as succinoxidase, D- and L-amino acid oxidases, glutaminase, tryptophan peroxidase, and arginase. Usually the cell-free activity is somewhat lower, until it is learned how to retain it. A discrepancy of 10-fold is a great rarity. My point was that the two activities were comparable, within an order of magnitude, and not dissimilar in a constant direction (suppressed in the cell) and not different by a factor of millions. I see no reason to invoke new and strange phenomena, as Barron would do, to explain these differences with different techniques, even in microbial systems. We met such an increase, though smaller than yours, with E. coli arginine decarboxylase (J. Bact. 58, 447 (1949)). The complexity here is our inability to decide what an enzyme does in vivo, as well as how fast it does it.

We will call Weinland's enzyme lactase. Thanks for the sobering influence. I have no reason to doubt his findings, despite the crudity of his methods and the possibility of bacterial contamination. The latter was not excluded in the controls with galactose, parenteral lactose, etc., but in those experiments the activity was not found. We have not yet repeated his work, but it was confirmed by Bainbridge by a better method, and by Abderhalden, for what his work is worth.

We have not found better evidence than that quoted that colchicine causes adrenal cortical hormone release. I am aware of its prompt pharmacological actions, but we did not feel justified in equating the "alarm reaction" with hormone release because it is not an accepted bioassay of the hormone.



W. Eugene Knox, M.D.